V. *Report of Progress in Pedigree Moth-breeding to Dec. 7th*, 1887, *with observations on some incidental points.* By Frederic Merrifield, F.E.S.

[Read December 7th, 1887.]

Plate V.

It will rest with Mr. Francis Galton to describe at the proper time and place the results of the experiments in pedigree moth-breeding which I have commenced for him, if they should be carried to a successful conclusion; but in the meantime I am encouraged by him to write a sort of report of the progress hitherto made, and I think it is possible that the facts already observed may throw light on some points that are frequently subjects of inquiry and discussion in entomological and other periodicals. There are many of these points on which I have noted facts that may hereafter prove useful; but there are not many on which the observations made have been carried far enough to justify me in occupying the Society with them, and as to these I bring them forward partly in the hope of receiving suggestions from investigators qualified to offer them by scientific training and a lengthened experience, to neither of which I have any claim.

Having obtained an abundant supply of *S. illunaria* (*bilunaria* of the 'Entomologist' list) much earlier than of *S. illustraria* (*tetralunaria* of that list), I was led to try more experiments with the former than I had at first intended. I determined, in particular, to try the effect of forcing, partly in the hope that if success attended these efforts the period necessary to obtain pedigree results would be much shortened, and partly because I thought it would be interesting to know the effect that would be produced by forcing a rapid succession of short generations on an insect which in the natural state has in temperate climates only two generations, one covering four or five months mostly warm, the other seven or eight months mostly cold, each of these naturally alternating broods presenting such differences in size,

colour, depth of hue, and, it is alleged, form, that until one was bred from the other the two were considered distinct species.

In describing my experience with the several groups successively experimented on I begin with those brought up under conditions most nearly resembling natural ones, as they will afford a convenient standard of comparison with such as were reared under more artificial circumstances. I therefore commence with those *illunaria* which were "sleeved" on growing trees. I am inclined to think that—except in the favourable circumstance that they were more effectually protected from enemies—the sleeved larvæ differed so little in their surroundings from wild-bred ones that they may be taken as fairly representative of the latter. I should, however, mention that there was one period of their lives during which nearly all the sleeved insects were subjected to a higher temperature than the natural one. In order to bring the moths out as closely together in point of time as possible, when the first moth appeared, the remaining pupæ were at once put into the forcing-box. I am not sure that this was necessary, especially with the summer brood of moths, for my experiments lead me to think that healthy individuals of this species, if kept in the dark, will live for ten days and more in summer, and for two or three weeks or more in colder weather, without any impairment of their functions, and only in rare instances will flutter so as to damage the tips of their wings enough to prevent convenient measurement.

Some preliminary explanations are necessary as to general treatment, and as to the sense in which I have used various expressions. My reason for being a little particular in these explanations is that any value such experiments as I am describing may possess depends entirely on a knowledge of the conditions under which they were tried. I have not knowingly burdened the narration with any statements, except such as seem to have some bearing or possible bearing on the results obtained. By "eggs," unless otherwise specified, I mean *fertile* eggs; and by the expression "fertile," as applied to the Selenias, I mean such as turn red, though many that go through the red stage and even the black one, which indicates that the young dark-skinned larva has been fully formed, often fail to hatch. As to the expressions

"larval" and "pupal" periods, I must explain that I found it impossible to observe, except on a few occasions, the actual date of pupation which, barring accidents, takes place inside a leaf carefully sewn together. But with the daily or almost daily, however brief, observation I was able to give, it was easy to see pretty well when a larva began to spin up, and consequently I have taken that time as the dividing line between the larval and the pupal periods. I found on several occasions, when the pupal period as thus defined lasted but eight or nine days, the larva remained in an unchanged condition for two days and more.

In my record I have found it expedient to note the period when "nearly all" had, as larvæ, spun up (*i. e.*, begun so to do), or, as moths, had emerged, because some 4 or 5 per cent., more or less, generally lagged behind the rest, from weakness of constitution I rather think. Excluding these laggards, I think the largest individuals of a brood were mostly to be found among or in point of time near to those that were longest in feeding up, and consequently in emerging. About 5 per cent. of the loss in my larvæ after I had first counted them after hatching may, I think, be ascribed to casualties, such as being squeezed or snipped or accidentally lost.

The pupæ were in all cases taken out of their cocoons and placed each in a separate chip box covered with black net, which was held in position by the rim of the lid, from which its top had previously been removed. These boxes stood on wire trays in crates and as the moths emerged were moved to crates kept dark by zinc covers standing in the cool room described later, near the window, almost always kept open, the sexes being in separate crates. I generally found the moths, especially *illunaria*, "out" when I came into the room in which they were kept, about 7.30 or 8 a.m., but some, perhaps 20 to 40 per cent., would come out during the day, rarely after 5 p.m. There is a very great difference between *illunaria* and *illustraria* in the resting position. The former rests with wings folded closely together over its back, as butterflies do. *Illustraria*, on the other hand, rests with the anterior edges of its fore wings at an angle of 60° or so to each other, the wings being all very much curved and the folds in them very wavy, and the abdomen brought into line with them, so that the insect has

somewhat the appearance of a curled leaf with the concave side upwards. It would be interesting to know the position in which the other English species of the genus, viz., *S. lunaria*, rests. At first I fed the moths from little pieces of sponge dipped in very thin syrup, but I gave this up, as it seemed to promote mouldiness, and I do not think the moths lived any the longer for it. I never saw them feed, but had little time for watching them.

When "nearly all" the moths had emerged they were measured on their under sides, the wings being folded together over their backs. The length of the fore wing was measured from its tip or extreme anterior point (B in Mr. Galton's figure, *ante*, p. 22). The other or shoulder extremity is not so easily ascertained or described, and at first the search for it gave me some difficulty; but after a certain amount of practice I found that when a strong light fell obliquely along the wing in the direction from the tip towards the shoulder, it brought out a little dark transverse crease, in some cases shortened almost to a point, between the root of the hind wing (which, viewed from the under side, of course overlies the fore wing) and the body, and this crease I made my other terminus, taking the precaution of always laying the insect to be measured in the same position. This was done by fastening on the surface of a sheet of cork two strips of the same at about five-eighths of an inch apart, so as to leave a shallow flat groove of that width between them, and laying the moth on its side in this groove, a thin wedge of wood sheathed with zinc being pushed along the groove so as to support the wings, especially their outer edges, and the wings being held down with the usual cork setting-bristle. A pair of screw-compasses was then taken, one leg fixed on the zinc at the tip of the fore wing that lies uppermost, the other leg adjusted to the crease by turning the screw, and the length was marked off on a millimetre scale. The habit of the Selenias to bend their wings backwards when at rest facilitated the task, but I found chloroform indispensable; applied in the form of vapour by a few drops on blotting-paper under a bell-glass just long enough to produce insensibility, it did not seem to hurt the insects in any way. The use of a pair of spectacles strong enough to bring my eyes to see clearly at five or six inches distance from the object was sufficient to enable

me, as I judge, to *estimate* differences amounting to the tenth of a millimetre, and I do not think all sources of error taken together would much exceed a quarter of a millimetre. It was not, however, until after I measured my sleeved *illunaria* on the 22nd July that I attained to this amount of accuracy, and therefore my earlier measurements must be taken as only approximate; but I think the *general* results are not far wrong. I should add that the "crease" cannot always be found, especially where the moth is very hairy; experience will tell the observer where it should be, and if the same person always measures, not much addition need be made to the percentage of error on account of the absence of the "crease." All measurements are of one wing only, so that the "expansion of wings" would be double the measurement given, plus about 3·5 mm. for the width of the body between the wings at the point measured. The "expansion of wings," however, measured from tip to tip of a moth set in the English fashion, would be about 1 mm. less than double the expansion of the single wing, owing to the inclination downwards and forwards.

After the moths had been measured, they were paired off in cylindrical muslin bags kept open by wire frames, each about 8 or 9 inches by 5; these bags, except where otherwise stated, were kept on a shelf outside the window of a cool room facing W.N.W., and protected from heavy rain; and there the moths laid their eggs, generally scattered over the muslin, and preferably in folds. I gave up inserting sprigs of the food-plant, as I found they rarely took any notice of them. The eggs, which will bear rough handling, were detached by hand or by the back of a knife, &c.

Nearly all the facts recorded are from my own personal observation, as I did not leave home for more than three or four days at a time, except during the last ten days of September and less than a week at the end of October, and on these occasions I had an efficient *locum tenens*, who had acted as my assistant at other times.

I have a more or less full record in most cases of the number of eggs laid, the number hatched, the number of moths that pupated and of moths of each sex that emerged, with dates and measurements, all of which may be useful for reference before the experiments are brought to a close, and which will, I hope, be dealt with by

Mr. Galton, so far as they bear on his studies in heredity. In this paper I propose only to give a *résumé* of facts observed in the different broods, for the information of those who are interested in investigations of this nature. All the moths have been preserved, and are labelled, and I have brought with me some specimens of the various broods. It will be seen that in the case of the forced *illunaria* I have had to do with as many as five successive generations in the year. In speaking of a "generation" or "brood" I reckon it as beginning with the egg; in this sense I have had to do with the first generation only in its latest, winged, stage. It is proper to remember that the succession of broods would have been still more rapid than it has been had I paired off the moths as soon as I had a couple; the delay necessary for making a selection added about a week of time. I have had actual experience of the following periods—egg 7 days, larva 16 and pupa 8 days, pairing and laying 2 days, total 33 days; and I am satisfied that it would be possible to run a generation through from egg to egg in 35 days.

I exhibit a diagram,* which will be a guide to the observations that follow, and will save much detailed description. It shows the connection of all the broods reared, and marks the extreme duration of life in the egg, larva, and pupa, and the duration of life in the moths from the time that the first appeared until the selection was made for breeding from. The moths so selected generally lived from 7 to 14 days; the others were killed and preserved.

The experiments with S. illunaria.—The spring of 1887 was, as all will remember, a singularly cold and backward one. No *illunaria* were taken for me till 12th April. I bred from two females taken near Brighton on the 29th April and 2nd May respectively by Mr. A. C. Vine, who kindly gave them to me, and from two females taken on the 2nd May in the New Forest by Mr. Charles Gulliver, of Brockenhurst. They laid from 48 to 133 eggs each. Some of the eggs laid by them were used for preliminary trials. There were 271 left. I divided each of the four batches into three, and, mixing together one-third from each batch, obtained three lots of 90, 90,

* See Plate V.

and 91 eggs for sleeving, bottling, and forcing respectively.

Sleeved Illunaria.—*Preliminary.* — The eggs were placed in sleeves on young birch-trees not exceeding three feet in height. Though the trees were only planted last December they were in so good a condition for moving, and were so carefully removed, that the summer foliage seemed scarcely checked by the operation. My back garden, in which they were planted, is a cool one, shaded by a tall house on the E.S.E., and by a wall of five to six feet along the S.S.W. side, and the trees were mostly planted very near this wall. At midsummer they received no sunlight except between 10.30 and 1.30, and during most of this interval it was partial. These retarding conditions were perhaps somewhat counteracted by the protection afforded by the sleeve from wind and from all but heavy rain. The sleeves were made of "Victoria lawn," kept from collapsing by three split cane-rings sewn in. There can be no doubt that sleeving is the least troublesome way of feeding larvæ that require no earth; the only trouble I have found is in shifting them while young from one sleeve to another, but any loss in the process was prevented by spreading a slit newspaper on the ground below. My provision of growing leaves being small, I frequently supplemented it with fresh-cut twigs of birch, willow, or occasionally rose, dropped into the sleeve. Both *illunaria* and *illustraria* are very accommodating feeders; they will eat most forest-trees and shrubs, including brambles, and will also eat evergreen honeysuckle (*L. brachypoda*), the variegated Japan honeysuckle, and the small-leaved evergreen *Cotoneaster*; and three or four out of a score survived a diet of ivy. Mine seemed to prefer willow to everything else. When autumn came they appeared to like the leaves that were beginning to turn yellow as much as those that were still quite green. In the autumn my supply of growing foliage became exhausted, and, when the larvæ had mostly entered on their last skins, I moved them into breeding-cages: these had glass tops and ends, and finely perforated zinc sides, and the food in them stood in bottles of water. Little as the ventilation was I found that in the dry weather, of which we had so much

last summer and autumn, the food in them dried up very rapidly, and I provided the sides with coverings of varnished paper. The effect of these was that water usually stood in drops about the glass inside and sometimes ran down the sides, but the larvæ seemed none the worse for this. The dwarf sleeved trees were protected from birds, &c., by a cylinder of ¾-in. wire-netting, with a hinged top of the same, and from slugs by an outer ring made of a strip of perforated zinc 6 in. wide, any slugs within the ring being caught by greased cabbage-leaves. When my first sleeved brood was reared I put the eggs in the sleeve to hatch, but I afterwards adopted the plan of hatching them indoors, and putting the young larvæ in the sleeve when a few days old. I judged it best not to crowd together young larvæ of different ages; I am not sure the larger ones do not under such circumstances sometimes eat the little ones. By the time they have changed their second skins no naturally solitary larvæ can be more tolerant towards one another. Having made these explanations, I will shortly describe what happened to each successive brood, referring also to the tabular statement appended.

Second generation (first summer brood).—From the 90 eggs I reared 34 male and 23 female moths, together 57, none of them being cripples. The eggs were rather more than three weeks hatching; the larval period averaged 38 days; the pupal period of the first moth that emerged was 13 days. The pupæ were forced from the time the first moth appeared—15th July—and the last came out 25th July. May and the early part of June were very cold and dry. I paired off 9 couples, 7 of which laid fertile eggs. I bred from the largest pair (A) a medium-sized pair (M) and the smallest pair (Z).

Third generation (A 1, M 1, Z 1).—These eggs hatched in 7 or 8 days; the larvæ averaged 50 to 60 days in feeding up. I have obtained from them the following pupæ, now passing the winter out-of-doors, of A 1, 101; of M 1, 64; of Z 1, 60. As the sleeved food was in danger of falling short, on the 13th September, when a few were beginning to spin up, I transferred the larvæ from the sleeves to breeding cages; and on the 15th October these breeding cages were brought indoors to hurry on the remaining larvæ before their food-supply should fail. All were in pupa by the 25th October.

Bottled Illunaria.—*Second generation.*—These were brought up on cut food in Bordeaux plum bottles, covered with muslin, plate-glass being laid over the top and slid away when the moisture inside the glass was excessive. The 90 eggs were three weeks in hatching; the larval period averaged 30 or 31 days, the pupal 14 days. I bred 31 males and 32 females, together 63; no cripples. They were distinctly larger than the sleeved ones. When the larvæ were about half-grown (on 18th June) I transferred half of them to an outdoor breeding-cage. The only difference I found in the moths so treated was that they were about two days later, and were smaller, *viz.*, the male averaged 17·60 instead of 17·70, the females 19·00 instead of 19·50. The weather was so warm most of the time that there could have been little difference in *temperature* between the two batches; but it was very *dry* weather out-of-doors, while in the bottles a moist atmosphere prevailed. I did not think it necessary to continue this brood.

Forced Illunaria.—*Preliminary.*—The forcing boxes were two, their inside dimensions about 2′ 4″ by 1′ 8″, and 2′ in depth. They were of wood, with glazed lids set on a slight inclination forwards, and ventilation capable of being closed, and were warmed at the bottom by a zinc cistern, under which was a gas-jet. One had glass also in front and partially at the ends. The temperature was generally from 70° to 80° Fahr., but occasionally (more especially when the sun shone into the room in the afternoon) it rose to 90°, and sometimes at night it fell to 60°. In the summer it was generally some 15° higher than the air of the room. Until the larvæ were about half-grown, and sometimes till they had spun up, the forced larvæ were brought up on cut food in bottles, or else in glass cylinders having a sloping sheet of muslin at the bottom, with a hole in it for the neck of a bottle containing food. When half-grown they were generally transferred to breeding-cages placed in the forcing-box; in both cases the atmosphere was quite a moist one. The forcing did not begin till 28th May, when the eggs laid by the wild-bred moths were on the point of hatching.

Second generation.—From the 91 eggs I reared 25 male and 33 female moths, together 58; no cripples.

The hatching (not forced) lasted about 3 weeks, the larval period 18 or 20 days, the pupal 8 or 10 days. The moths were larger than the sleeved ones, but not so large as those that were bottled. On 3rd July I paired off two of the largest (A), 5 of medium size (M), and 2 of the smallest size (Z); and from all of these, except one M and one Z, I had fertile eggs, which I bred from as follows:—

Third generation (eggs not placed in the forcing-box till earliest of them about to hatch).—The 3 broods did not vary much in their rate of progress—the M's were 2 or 3 days behind the A's, the Z's 2 or 3 days later still: the larval and pupal periods together were about the same as in the second generation. From 205 A 1 eggs I bred 68 male and 61 female moths, together 129, six cripples; from 115 M 1 eggs, 35 males and 53 females, together 88, one cripple; from 107 Z 1 eggs, 16 male and 14 female moths, together 30, 3 of them cripples, and two so weakly that they died before they could be paired: many of the Z 1's died as larvæ. The A 1's comprised the largest I had yet bred; I did not average them: the M 1's (averaged by taking every alternate one of each sex in the order of emergence) were slightly larger than the average of the preceding generation: the Z 1's considerably smaller. I paired off 4 of the largest couples among the A's, 4 average couples of the M's, and 10 couples of the Z's. None of the A's or Z's laid a fertile egg: 3 out of the 4 M's laid fertile eggs, and from one of these pairs, paired 16th August, I had 210 eggs, which I bred from as follows:—

Fourth generation, M 2.—These were not only slower, but straggled more in their feeding up and emergence than the earlier forced generations had done. The first spun up 23rd September; by 8th October nearly half had done so; and on the 1st November all had done so except two, which soon after died. Many larvæ died in pupating, and a few before. I have some reason to think this was owing to their having been made too hot at one time. The first moth appeared 2nd October; by the 3rd November 60 were out, and on the 7th the last appeared; but 3 or 4 are still in pupa, one or two of them certainly being alive. 36 are males and 25 females; 3 were cripples, and 3 more died before they were paired off. The hatching occupied about 10 days

(eggs not in the forcing-box till the first eggs were about to hatch), the larval period ranged from 26 to about 59 days; the pupal period seems to have been about 12 days. After the early alarm they were kept rather cooler than the preceding forced generation had been, as, with the advent of cooler weather, I found it difficult to keep up a high temperature without making the bottom of the forcing-box very hot. The average size had again risen on that of the preceding generation. On 23rd October I paired off 6 couples, keeping them in the forcing-box, and 4 of them were fertile. The largest pair laid 170 eggs (called M 2, A 1), the medium-sized 210 (called M 3), the smallest 80 (called M 2, Z 1).

Fifth generation.—About 86 of the first, 169 of the second, and only 24 of the third hatched. The numbers are now about 82, 151, and 21 respectively. I am feeding them up on rose and evergreen honeysuckle, and the most forward are nearly full-grown, as will be seen by the living specimens I exhibit. I have made an improvement in my forcing-box, so that I can keep up a more equable temperature without danger of roasting those which are near the cistern, and I keep it at about 70° to 80°.

Illunaria.—*General results.*—Without venturing any opinion on many of the questions suggested by an examination of the facts above detailed, until more facts have been accumulated, I may advert to a few of them. It seems to be established that *S. illunaria* forces well, and there is evidence that the average size of forced specimens is larger than that of the insects reared on growing trees, and tends for a time to increase from generation to generation, notwithstanding close interbreeding. I am not satisfied that the fertility has been diminished by the process of forcing; but it does at present appear as if extremes in size, especially in the direction of smallness, have a tendency to be sterile, and I think it prudent to select the breeding pairs from some point quite short of either extremity in the scale of size. There is another fact established as to the summer broods of *illunaria*,—all of which that I have reared, I need hardly say, are in appearance of the summer type, *Juliaria*,—*viz.*, that, in accordance with the usual rule with the *Geometræ* inhabiting this

country, the female on the average is larger than the male, and decidedly so. This will appear clearly by the tabular statement I refer to. My own personal experience, which is confirmed by trustworthy information I have lately received from several quarters, is that in the spring brood the case is reversed, so that the male is decidedly the larger; at all events, it seems certain that the spring female has no excess of size approaching to what she shows in the summer brood. In this connection I venture to call attention to the following points :—(1), of 272 *Geometræ* described in Stainton's 'Manual,' only 16 are recorded as appearing in the five months from November to March; (2), *illunaria* in its spring emergence is one of them; (3), of the remaining 15, 9 have apterous or quasi-apterous females (there being only two other apterous females among the 272, and these two appear in April and October respectively); (4), another of the 15 (*H. pennaria*) has the wings of the female strikingly smaller than those of the male. Is it possible that the relative size of the female in the spring emergence of *illunaria* is a step towards the condition of apterousness, or, it may be, a remnant of it? So far as I have had means of judging, *illustraria* and *lunaria* do not show such a difference between the sexes according to the season of emergence, but their spring broods are much later than those of *illunaria*, which (unless *Tephrosia laricaria* (*biundularia*), another of the 16, of which I know but little, resembles it in this respect), is unique among double-brooded English *Geometræ* in producing its early brood in a winter month.

S. ILLUSTRARIA. — Mr. Barrett kindly sent me eggs from a female taken in Norfolk in May, and Mr. Gulliver, of Brockenhurst, supplied me with some larvæ beaten in the New Forest. From these two sources I bred 9 males and 12 females, and, though the variety in size was not very great, I selected a large (A), medium (M), and small (Z) pair, the eggs from which I sleeved; and from them I have three batches of hybernating pupæ, *viz.*, A 116, M 103, and Z 78. I reared several mixed broods in the forcing-box, with some remarkable results, which I hope to follow up.

Concluding Remarks.—I shall be very glad if the account I have given of the experiments with the Selenias, and of the ease with which they can be bred, should lead others better qualified than I am to take up the subject; and I shall be glad to supply eggs of any race bred. The remarkable changes which the larvæ undergo in appearance, attitude, and habits, so well described by Mr Poulton; the perfection to which the imitation of jagged twigs has been developed in them; the great variation in size of individual moths, especially in the spring brood, and in shape; the richness and variableness of shading and colour in the wings, and their unusual positions when at rest, apart from other points to which I have already called attention, make them a very remarkable group, and they ought to have an interesting family history. The experiments I am trying with *illunaria* and *illustraria* will leave abundant scope for other investigators who may direct their attention to these two species, and a very interesting species, *S. lunaria*, remains. *Tephrosia laricaria*, which is stated to resemble *illunaria* in having an early spring and summer emergence, and in the smaller size and different appearance of the latter brood, would also be an interesting species to work up. As to *illunaria* and *illustraria*, may I suggest that practical entomologists would be promoting the investigation by preserving any specimens they may meet with next spring, or a fair sample of them, for comparison with the numbers I expect to bred? I should be particularly obliged by being afforded any opportunity of seeing, and, if judged expedient, breeding from, specimens of either species from Scotland or Scandinavia, where they are stated to be single-brooded, or from Ireland, Wales, or Central or Southern Europe.

[*Note as to Measurement.*—I find it is practicable, without piercing the insect, to measure the *expansion* of wings of the chloroformed insect by setting it temporarily, with cork setting-bristles, on a *flat* setting-board covered with paper ruled in square millemetres, and after trial I recommend this mode decidedly as the more safe and certain in its results. The tips of the fore wings should be as widely separated as possible, so that the front edges of these wings will be nearly in a straight line.]

TABULAR STATEMENT OF FERTILE EGGS LAID, AND MOTHS REARED, WITH MEASUREMENTS.

Eggs.	MOTHS.					Larval & pupal period. Days.
		Largest.	Smallest.	Diff.	Average.	
	First Generation.					
		Wild.				
	♂ 1				20·20*	
	♀ 4	22·40	18·10	4·30	20·20	
	5				0·0	
	Second Generation.					
		Sleeved.				
	♂ 34	18·70	16·70	2·00	17·35	
	♀ 23	20·30	16·30	4·00	18·24	
90	57	1·60	·40		·89	51
		Bottled.				
	♂ 31	18·70	16·60	2·10	17·65	
	♀ 32	20·70	18·10	2·60	19·26	
90	63	2·00	1·50		1·61	45
		Forced.				
	♂ 25	19·00	15·90	3·10	17·54	
	♀ 33	20·80	17·90	2·90	18·95	
91	58	1·80	2·00		1·41	30
	Third Generation.					
		Forced. A 1.				
	♂ 68	19·60				
	♀ 61	21·10				
228	129	1·50				30
		Forced. M 1.				
	♂ 35				17·88	
	♀ 53				19·20	
115	88				1·32	32
		Forced. Z 1.				
	♂ 16	16·90	15·40	1·50	16·15	
	♀ 14	18·30	15 60	2·70	17·57	
107	30	1·40	·20		1·42	34
	Fourth Generation.					
		Forced. M 2.				
	♂ 35	19·80	17·00	2.80	18·41	
	♀ 26	20·90	18·20	2·70	19·40	
210	61	1·10	1·20		·99	52

* Average of five males taken in spring, 21·50; of five females then taken, 19·90; difference in favour of male, 1·60. In all the later generations the difference is ·89 to 1·61 in favour of female.

Explanation of Plate V.

The explanation of this Plate will be found at p. 128.